It will be seen that the differences of N.P.D. are comparatively large, but that there is a decided sequence, which is more obvious from Table III., where the means of the preceding results are taken in groups of five.

TABLE III.

Means of Differences arranged in order of N.P.D. and taken in groups of 50 *Stars.*

Approx. Mean N.P.D. of group.	Difference of R.A. Sydney−Cape ×sin N.P.D.	Difference of N.P.D. Sydney−Cape.	Correction of the form +1″·576×sin *zd* Sydney.
° ′	s	″	″
74 27	+0·010	+0·83	+1·20
97 22	+0·024	+0·69	+0·73
118 41	+0·061	+0·09	+0·14
123 56	+0·121	+0·01	−0·00
134 24	+0·109	−0·30	−0·29
152 24	+0·074	−0·89	−0·74
General Mean	+0·055	+0·09	

The latitudes of Sydney and of the Cape happen to be nearly identical, each being approximately 33° 54′. The N.P.D. of the fourth group in Table III. is therefore nearly that of the zenith. From the mean of this group in N.P.D. it may be inferred that the difference of the adopted latitudes of Sydney and of the Cape is not far from the truth. The means of the other groups seem to point to the want of an instrumental correction, depending on the zenith distance, to the observations of one or the other catalogues. No correction of this kind was applied to the Sydney observations, but determinations of horizontal flexure by the collimators gave a mean value for the flexure constant of 1″·576. The values of a correction of the form $+ k \sin z.d$ with the above value of k, applicable to the Sydney observations of zenith distance, and therefore applicable with changed sign to these differences, are given in the last column of Table III.

Remarks on Three Volumes of Sun-spot drawings presented to the Society. By Rev. F. Howlett, M.A.

I have the honour of requesting the Society to accept what will, probably, be the *final instalment* of my solar drawings. Many thousands of spots great and small are therein depicted with the greatest care and, let me say, conscientiousness, of which I was capable.

The series, more or less continuous, extends over a period of about five-and-thirty years, and I may truly say the work has been a labour of love and of the deepest interest. I imagine (if I may say so without arrogance) that no such series of hand-

drawings, showing the minute details of the configurations and changes of the solar spots, exists.

If in this respect I am in error, I shall loyally yield the palm of superiority to any other brother observer, whether in England or any other part of the world.

Since the application of the marvellous and invaluable art of *photography* to celestial phenomena of every description, I have sometimes wondered whether, with the view of advancing any real knowledge of the Sun, it was worth my while to continue such a series of drawings; and this has, for one reason, been the cause of very considerable gaps in the record. But encouraged by the plea on behalf of hand drawings of solar phenomena, made in an address to the members of the Royal Institution, a few years ago, by the late admirable and lamented Father Perry, I persevered in the work. Moreover, whilst an almost unbroken daily record of the solar disc on a small scale is preserved at our great National Observatory at Greenwich, it is only, I believe, at rare intervals that astronomy is enriched and instructed by such splendid details of the photosphere, faculæ, and solar spots as are furnished by the photographs of Professor Janssen more especially; though, even in them, the sharp distinction existing between the umbræ and penumbræ is by no means satisfactorily shown. There is sometimes, in fact, no difference manifested at all.

I would now take this opportunity of saying that the main object I have had in view in continuing this series of drawings for so many years—through four periods of maximum solar spot activity and three of minimum activity—has been to test the theory first put forth by Professor Wilson, of Glasgow, as recorded in the *Philosophical Transactions* for the years 1774 and 1783. You all know what that theory is, as maintained in the *Transactions* with what was intended to be the most scrupulous care and attention, though, I cannot but now feel certain, with very imperfect instrumental means and appliances.

What these were is fully described in the *Philosophical Transactions* to which I have just alluded. Wilson's theory is, that owing to the way in which (according to his ideas) the penumbra in a spot *shelves down* towards the umbra of the same, the portion of the penumbra nearest to the Sun's centre will grow perspectively narrower and narrower, causing the umbra, which might be really *central* in the spot, to appear no longer central, but to lie close to the inner edge of the spot.

Now this, of course, *would* be the case if the spots were of that deeply depressed *crateriform* nature which almost all writers on physical astronomy have described them as possessing, but which my observations, made under most charmingly perspicuous conditions, have in almost every instance refuted. Some few spots, indeed, when lying close to the Sun's limb have appeared to favour slightly the Wilsonian hypothesis; but, as Mr. Turner (now Savilian Professor at Oxford) remarked to

me last year in a letter, "one ought to know the *history* of a spot," meaning, "had it been duly ascertained whether or not the umbra of a spot under discussion to test the theory was *really central* or otherwise suitable for such an investigation?" And oftentimes I have found that such a knowledge of their history refuted the testimony to the Wilson theory which an isolated observation seemed to afford. Several such instances will be found in the eight volumes of my drawings.

It is, I believe, generally known that my method of observing the Sun is by projecting the solar image on a screen, placing the latter at such a distance from the eye-piece of the small refractor of 3 inches aperture as shall, with a magnifying power of 80 linear, produce an image of 32 inches in diameter, or about one inch to one minute of celestial arc, though I frequently scrutinise the spots with higher powers producing an image of twice that diameter, as well also as by direct vision with the aid of a Dawes' solar eye-piece.

I cannot too strongly recommend this use of a screen and a small handy refractor, which, with a light shutter adapted to a chamber window, can in a moment be applied for an observation. I have here a photograph of such a shutter, with its appliance of vulcanised indiarubber for clasping the tube of the telescope and keeping out all extraneous light.

I will not on this occasion weary the Fellows with any general description of the contents of these volumes. Those who are interested in the subject of which they form illustrations can, at their leisure, honour them with their attention. There is an index of the spots at the commencement of each volume, wherein are specified those instances which distinctly militate against the Wilsonian theory, as well as those which seem to favour it. There are in the three volumes presented to-day no less than fifty of the former, and only nine of the latter, the majority, if not indeed all of them, being really of no real worth on the Wilsonian side, because, as I have said, the umbræ were shown not to have been *central*.

The theory, in fact, will simply stand or fall by careful and sufficiently prolonged observation, which few, unfortunately, have had the time or disposition to carry out. But, so far as they have been able to do so, both Professor Spörer, of Munich, the late lamented Father Perry, Mr. Cowper Ranyard, the late Mr. Whipple, of the Kew Observatory, and several of my own private friends who have witnessed the behaviour of spots near the limb of the Sun are satisfied that the Wilsonian theory that the spots are funnel-shaped depressions is entirely erroneous.

Moreover, as I stated in a paper which I read before the British Association last year at Nottingham, by the courtesy of the Astronomer Royal and the staff under his administration I was allowed to institute a comparison between the testimony afforded by my own drawings and that of the photographic pictures there accumulated; and I think I may say in

every case I was borne out in my contention that the spots can in no sense be called cavernous. Nay, I make bold to say, that instead of the penumbra of a spot possessing *shelving sides*, sloping down towards the umbra, it (the penumbra) presents a *convex* surface ; that is to say, a curve conformable to the general contour of the solar orb.

Observation leads me to the conviction that the photospheric stratum (save where faculæ occur) consists of merely one layer of the so-called "rice-grain" entities—say about 1000 miles at most in thickness—in close contiguity to the subjacent penumbra ; which, again, can possess no greater thickness than the photosphere, or the umbræ (whose depth I do not profess to estimate) could not be seen *central* in the penumbræ (as I assert they do) when very near the limb. By "very near" I mean not more than some 40 or even 30 seconds only of arc.

Lastly, in contravention of statements put forth on high authority, no spot, however large, or supposing it to have ever so shelving sides, could possibly present the appearance of a *notch* when passing round the limb, but only the slightest possible *flattening* of the sphere at that point, unless indeed the spot were accompanied by a most abnormal and extraordinary piling up of facular matter north and south (but not at the same time east or west) of the penumbra. But this I have never myself witnessed, save in one rather doubtful case on 1862 August 4, as shown in sheet No. 86 of vol. ii.

The appearance of a notch has occasionally been seen in a *photograph* ; but that was simply in consequence of the lack of sufficient power in the already degraded light near the limb to depict the yet more degraded dusky spot.

The Annular Eclipse of the Sun 1894 *April* 6.

By C. Michie Smith.

The first contact was lost through clouds, but the last contact was well observed, as follows :—

	M.M.T. h	m	s
Prof. R. Ll. Jones	21	17	43·8
C. Michie Smith	21	17	42·3
K. V. Sivaramiah	21	17	43·4

A number of photographs were taken during the eclipse with the hope of obtaining signs of the Moon against the corona, but without definite success. The eclipse was not annular at Madras ; but as the magnitude of the eclipse was 0·96, the reduction in light was very marked and the meteorological effects were considerable, as is shown by the following table :—